Experience Matters:

Human Capital and Development Accounting*

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July 10, 2012

Abstract

Using recently available large-sample micro data from 28 countries, we document that experience-earnings profiles in poor countries are flatter than in rich countries. This new fact motivates a development accounting exercise that adds to previous exercises by allowing the returns to worker experience to vary across countries. When the estimated returns to experience are accounted for as part of human capital using the standard method in the literature, human and physical capital can account for roughly two thirds of the variation in cross-country income differences, as compared to less than half in previous studies.

*We thank Mark Aguiar, Joseph Altonji, Paco Buera, Anne Case, Francesco Caselli, Thomas Chaney, Sylvain Chassang, Angus Deaton, Mike Golosov, Joe Kaboski, Nobu Kiyotaki, Luigi Pistaferri, Richard Rogerson, Todd Schoellman, David Sraer, Chris Taber, Chris Udry, David Weil and seminar participants at Princeton, the World Bank, Rochester, LSE, EIEF and EUI for their insights. We thank Xin Meng for generously providing us with extracts from the Chinese Urban Household Surveys.

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1 Introduction

Understanding the determinants of cross-country income differences is one of the central aims of development and growth economics. To this end, numerous studies have conducted development accounting exercises to assess the contribution of observable factors of production – namely, physical and human capital – to cross-country income differences. One of the main challenges in such accounting exercises is the measurement of human capital stocks. Klenow and Rodriguez-Clare (1997) and Hall and Jones (1999) first addressed this difficulty by constructing human capital stocks from country-level measures on educational attainment. Later studies added to this by taking other aspects of human capital into account. These include: schooling quality (Barro and Lee, 2001; Hanushek and Kimko, 2000; Hendricks, 2002; Schoellman, 2012), experience (Klenow and Rodriguez-Clare, 1997), and health (Weil, 2007; Shastry and Weil, 2003).\footnote{Barro and Lee (2001) construct quality of schooling measures by using student-teacher ratios and government spending on education. Hanushek and Kimko (2000) measure quality with test scores. Weil (2007) and Shastry and Weil (2003) account for health using data on adult mortality rates. In a related study on the relationship between growth and human capital, Bils and Klenow (2000) also construct human capital stocks taking into account the levels of both schooling and experience.}

As summarized by Caselli (2005) and Hsieh and Klenow (2010) in their surveys of the literature, these measures of human capital and standard measures of physical capital, account for less than half of the variation in cross-country income differences. In other words, more than half of this variation is accounted for by residual total factor productivity (TFP).

In this study we document a new fact, namely that experience-earnings profiles in poor countries are flatter than in richer ones. We then show that taking this fact into account in a standard development accounting exercise can significantly reduce the fraction of income gaps accounted for by residual TFP. Relative to existing studies, our innovation is to allow returns to experience to vary across countries. Our results show that cross-country differences in human capital due to experience are roughly as big as those due to schooling. Taking into account cross-country differences in returns to experience therefore roughly doubles the dispersion in human capital across countries. When we then conduct a standard development accounting exercise, physical and human capital taken together account for roughly two thirds of the variation in cross-country income differences as compared to less than half when experience is not taken into account. Put differently, experience matters for development accounting.
Our analysis also improves on past studies in using large-sample micro data from 195 household surveys for 28 countries that have only recently become available. These data provide several advantages. First, the large sample size – at least five thousand but often many more individuals per survey – allows us to estimate the returns to experience with minimal restrictions on functional form. Second, our data are derived from comparable sampling frames across countries. In particular, all our surveys are nationally representative, or representative of urban areas, and contain comparable data on labor income. Our cross-country comparisons are therefore likely more accurate than past accounting exercises that have used estimates for returns to schooling and experience based on data drawn from different sub-populations and different time periods across countries.  

Finally, for most countries we have surveys for two or more years available to us. We can therefore control (at least to some extent) for cohort effects or year effects in our estimates, and thereby check that our cross-sectional estimates of the experience-earnings profiles are not driven by, say, aggregate growth or improvements in birth cohorts over time. The main limitation of these data is that they are not available for all countries. Nonetheless our set of 28 countries comprises a wide range of income levels, with the United States, Canada and Switzerland at the high end and Bangladesh, Vietnam, and Indonesia at the low end. Thus, while we still lack data from many countries, our analysis does represent a sizeable fraction of the world’s total population and our results plausibly generalize outside the specific contexts for which we have data.

We begin our empirical analysis by allowing the returns to experience to vary fully flexibly for each additional year of experience. These fully flexible estimates show that experience-earnings profiles in poor countries are typically flatter than those in rich countries. This fact is robust to controlling for either year or cohort effects. The estimated experience-earnings profiles are also highly nonlinear and cannot be accurately summarized by a linear-quadratic Mincerian specification as is typically used in the literature (e.g. the studies by Psacharopoulos, 1994). At least a quintic specification is needed to provide an accurate approximation to the fully flexible specification.

2 For example, see the studies by Psacharopoulos (1994) and Krueger and Pischke (1992) among others that Bils and Klenow (1998) extract their cross-country estimates shown in their Appendix Table B from.

3 The combined population of the countries for which we have at least one survey amounts to 66% of the world population.

4 This is in line with Murphy and Welch (1990) who argue that a quartic specification is needed to match age-earnings profiles in the US and that the traditional linear-quadratic Mincer specification provides a poor fit. The parsimonious quintic functional form we use has the advantage that it can be easily replicated in future studies even if the sample size of the data is limited.
This, together with data availability, may partly explain why our empirical finding that experience-earnings profiles are flatter in poor countries has (to our knowledge) not been documented before.

Following the development accounting literature, we make the standard assumptions that workers are paid their marginal products of labor and that human capital is valued in efficiency units.\(^5\) Under these assumptions, flatter experience-earnings profiles in poor countries reflect less human capital accumulation. To assess the quantitative importance of taking our country-specific estimates of the returns to experience into account, we conduct a series of accounting exercises. First, we calculate the part of human capital due to experience and show that this is positively correlated with income. Then, we show that the same is true for human capital due to both experience and schooling. Finally, we show that allowing the returns to experience to vary across countries increases the contribution of physical and human capital to cross-country income differences from less than half to roughly two thirds. Our results are robust to different ways of measuring the returns to schooling and again to controls for time or cohort fixed effects. A useful way of thinking about our exercise vis-à-vis existing development accounting exercises is as follows. Traditional exercises eliminate all productivity differences across countries and ask what fraction of cross-country income differences can be accounted for with quantities of observable factors of production only.\(^6\) While keeping final goods aggregate production functions the same, we instead allow human capital production functions to vary across countries and empirically identify what amounts to cross-country TFP differences in human capital production.\(^7\)

Having documented that experience-earnings profiles in poor countries are flatter than in rich countries, and having argued that this is important for development accounting, we explore some potential explanations for this fact. We first ask to what extent our finding can be explained by cross-country differences in the composition of the labor force across, say, sectors and occupations. But we find that such composition effects explain only a relatively small fraction of cross-country differences in returns to experience and experience human capital stocks.\(^8\) What then explains the

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\(^{5}\)We discuss potential violations of this assumption and possible departures from it in more detail in the main text.

\(^{6}\)See the beginning of the introduction for references to past development accounting studies.

\(^{7}\)In this sense our exercise splits up the "black box" that is aggregate residual TFP into two smaller "black boxes": residual TFP in final goods production and TFP in human capital production. We believe that this is a useful exercise because our empirical evidence provides more structure for theorists aiming to explain cross-country income differences.

\(^{8}\)That composition effects do not seem to be an important driver of cross-country differences in returns to experience is true at the level of disaggregation we are able to pursue. This finding may be overturned with a more fine-grained
flatness of experience-earnings profiles in poor countries? We argue that there is a class of theories that is consistent with this finding: its common feature is that TFP and experience human capital accumulation are complementary, meaning that low TFP in poor countries depresses the returns to the accumulation of experience human capital and hence leads to flat experience-earnings profiles. An example is Manuelli and Seshadri (2010) but the precise mechanism could, in principle, be different.\(^9\)

Our study is most closely related to two studies that conduct development accounting exercises and allow the level of – but not the returns to – experience to vary across countries. Klenow and Rodriguez-Clare (1997) find that accounting for experience reduces the relative level of human capital stocks in poor countries and therefore, reduces the explanatory power of human capital for explaining cross-country differences. This finding is due to the fact that the positive effect of longer life expectancy and later retirement leading to higher levels of experience in rich countries is dominated by the negative effect of the fact that workers in rich countries obtain more schooling, which causes them to enter the labor force later, and hence to have less experience. Our paper differs from theirs in that we not only allow the level of experience to vary across countries, but also the returns to experience.\(^{10}\) Bils and Klenow (2000) conduct a similar development accounting exercise allowing the level of – but not the returns to – experience to vary across countries, though this is not the main focus of their paper.\(^{11}\)

Our argument is that cross-country differences in the steepness of experience-earnings profiles are due to cross-country differences in human capital accumulation.\(^{12}\) A large existing literature decomposition.

\(^9\)In these theories, human capital amplifies cross-country TFP differences which are the root cause of cross-country income differences (see also Erosa et al., 2010). This argument also highlights one important aspect of our study: our main result is that human capital can explain a substantially larger fraction of cross-country income differences than in previous studies, and therefore that TFP is less important. However, this is true only in an accounting sense and TFP (or other factors) may still be the root cause of cross-country income differences.

\(^{10}\)The data used in the two studies also differ.

\(^{11}\)While Appendix B of their paper reports country-specific estimates of the returns to experience obtained from other studies, their accounting exercise simply imposes the average of these estimates for all countries, in the spirit of keeping human capital production technologies the same across countries as discussed earlier in the introduction. The working paper version (Bils and Klenow, 1998) actually examines country-specific returns to experience in more detail, but it does not conduct a levels accounting exercise. It instead shows that these country-specific returns to experience do not vary systematically with countries’ income growth rates. We have independently found that Bils and Klenow’s country-specific returns to experience also do not vary systematically with income levels, which is in contrast to the finding in our paper. The most likely causes of the difference between our estimates and those that they use are that we allow the returns to experience to vary fully flexibly across experience levels, and that our data are more comparable across countries. We discuss this in more detail in the paper.

\(^{12}\)In our theoretical framework this follows directly from the assumption that workers are paid their marginal product of labor.
has examined experience-earnings profiles for the United States and has asked to what extent these reflect human capital accumulation over the lifecycle as opposed to other factors such as job shopping or job seniority. For example, a recent study by Altonji et al. (2009) finds that human capital accounts for most of the growth of earnings over a career and that job seniority and job mobility play decidedly smaller roles. The findings of this literature therefore lend some additional credence to our argument.

Our conclusion is that human capital can therefore account for a larger fraction of cross-country income differences than the conventional wisdom prescribes. This is in line with the conclusion of Mankiw et al. (1992) but for completely different reasons. Another more recent study with a similar conclusion is Jones (2011), but this again for different reasons. Finally, another recent paper that argues for the importance of human capital is by Manuelli and Seshadri (2010). As already mentioned, their mechanism is in fact consistent with our empirical evidence, a point we will return to later.

The paper is organized as follows. Section 2 describes the data. Section 3 presents a simple model to motivate the empirical exercise and estimates returns to experience across countries. Section 4 uses these returns to experience to calculate implied human capital stocks and conduct our development accounting exercise. Section 5 examines potential explanations for our empirical finding, and section 6 summarizes our results and offers concluding remarks.

See for example the decomposition of experience-earnings profiles in their Figure 1. Other studies, for example Topel and Ward (1992) and Bagger et al. (2011), have argued that the contribution of job search is somewhat larger than that postulated by Altonji et al. (2009). But all of these studies agree that human capital accumulation is the most important source of wage growth at least in the early phase of workers’ careers (which is also the phase in which the cross-country differences in returns to experience that we have documented are most pronounced).

Mankiw et al. (1992) proxy human capital with schooling attainment and do not take into account experience human capital. See also the criticisms in Klenow and Rodriguez-Clare (1997) and Bils and Klenow (2000) that their implied returns to human capital accumulation are not consistent with microeconometric evidence from Mincer regressions.

Jones points out that one critical assumption of traditional development accounting is that different skill types are perfect substitutes in production and argues that relaxing this assumption can substantially amplify the role of human capital in accounting for cross-country income differences (and even close the entire income gap between rich and poor countries if high and low skill types are sufficiently complementary). Our work is complementary but differs in an important dimension: we argue that even under the assumption that different skill types are perfect substitutes in production, taking cross-country differences in experience human capital into account can make a significant difference for development accounting exercises.
2 Data

Our analysis makes use of large-sample household survey data from a set of 28 countries. The surveys we employ satisfy two basic criteria: (i) they are nationally representative, or representative of urban areas, and (ii) they contain data on labor income, and for at least five thousand individuals. We make use of multiple surveys for each country whenever that data is available to us, and end up with a total of 195 surveys spanning the years 1960 to 2011. The complete list of countries and data sources we employ is available in Section A.1 of the Appendix.\footnote{We made an attempt to find data for every country in the world with a population greater than one million people. We plan to add surveys for several countries that we are still in the process of obtaining.}

Our set of countries comprises a wide range of income levels, with the United States, Canada and Switzerland at the high end and Bangladesh, Vietnam, and Indonesia at the low end. The combined population of the countries for which we have at least one survey amounts to 66% of the world population. Thus, while we still lack data from many countries, our analysis does represent a sizeable fraction of the world’s total population.

Our main analysis makes use of individual-level data on age, years of schooling, labor income and hours worked. We restrict attention to individuals between fourteen and sixty years of age with positive labor income and non-missing age and schooling information. In all surveys, we impute years of schooling using educational attainment data. In the majority of surveys, we measure labor income as monthly wages or salaries from both primary and secondary jobs. Similarly, in the majority of surveys, we measure hours as the actual hours worked at both primary and secondary jobs. Appendix section A.1 details the few cases when our labor income or hours measures differ; in some countries, for example, income and hours are reported only for the individual’s primary job.

We define potential experience as \( \text{experience} = \text{age} - \text{schooling} - 6 \) for all individuals with 8 or more years of schooling, and \( \text{experience} = \text{age} - 14 \) for individuals with fewer than 8 years of schooling. This definition implies that individuals begin working at age 14 or after they finish school, whichever comes later. The rationale for this choice is that we find that very few individuals before age 14 have positive wage earnings in any of our countries. In Section A.3 of the Appendix we give this argument in more detail, and present our main results under several alternative definitions of potential experience.

We define an individual’s wage to be her labor income divided by her hours worked.
majority of our surveys we observe hours worked directly, over the last week or some recent reference
week. In countries without hours worked data, we impute an individual’s hours as the average hours
across all other countries for that individual’s experience level. Section A.1 of the Appendix details
which surveys have hours worked information and which few are imputed.\footnote{We find that our results are robust to dropping countries without hours data, and substituting earnings for wages in countries without hours data.}

For most countries we have surveys for two or more years available to us. For these countries we
can control (at least to some extent) for cohort effects or year effects in our estimates. An important
limitation here is that, in most of our countries, our data covers a much larger range in terms of
birth cohorts than in terms of time (e.g. calendar years.) The reason is that most of our surveys
are from 1990 or afterwards. We return to this limitation later in the paper. In any case, as we
show later, our findings are largely similar when controlling for cohort or year effects as when not
controlling for either.

Finally, an important limitation of our analysis worth noting is that it excludes self-employed
workers, which form a substantial fraction of the workforce of developing countries (see e.g. Gollin
(2002) and the references therein.) We have two main rationales for excluding self-employed in-
dividuals. First, a lot of income from self-employment accrues in principle to the household, not
the individual. In many agricultural households, for example, all family members provide labor on
the farm and then share the proceeds of any output produced. Second, self-reported income for
the self-employed is often uninformative about the actual value of their output produced, as survey
respondents either misreport or are unable to accurately report their income.\footnote{According to Deaton (1997), it is extremely difficult to obtain accurate measures of income from self-employment unless one records the quantities and prices of each input used and output produced in production (such as done in the World Bank’s Living Standards Measurement Surveys.)} Thus, while in
principle we would like to include the self-employed in our analysis, measurement issues in practice
render that a task well beyond the scope of the paper.

3 Returns to Experience Across Countries

3.1 Conceptual Framework

A simple model of human capital, similar to that proposed by Bils and Klenow (1998), motivates
the empirical estimation. Human capital of individual $i$ who is a member of cohort $c$ at time $t$, $h_{ict}$,
depends on schooling, $s_{ict}$ and experience, $x_{ict}$:

$$h_{ict} = \exp(g(s_{ict}) + f(x_{ict})).$$  \(1\)

We further impose $f(0) = g(0) = 0$, meaning that we normalize the human capital of a worker with zero years of both schooling and experience to be one. In effect then, we define human capital as that part of earnings that is only due to schooling or experience. Further, assume that individuals are paid their marginal products in efficiency units of human capital up to a mean zero error term.$^{19}$ Hence an individual’s hourly wage is just his human capital times a skill price, $w_{ct}$, and an error term, $\varepsilon_{ict}$:

$$y_{ict} = w_{ct} h_{ict} \exp(\varepsilon_{ict}).$$ \(2\)

We allow the skill price, $w_{ct}$, to differ across cohorts and time periods:$^{20}$

$$w_{ct} = \bar{w} \exp(\gamma_t + \psi_c).$$ \(3\)

Substituting (1) and (3) into (2) and taking logs, we obtain

$$\log y_{ict} = \log \bar{w} + g(s_{ict}) + f(x_{ict}) + \gamma_t + \psi_c + \varepsilon_{ict}. \quad (4)$$

The main goal of our paper is to estimate the function $f(\cdot)$ and assess how it varies across countries. Our main empirical result is that this function is flatter than in poor countries than in rich countries. We first estimate (4) under the assumption that there are no cohort or time effects, $\gamma_t = \psi_c = 0$. We then show that results are robust to including cohort and time effects to the extent possible.

$^{19}$As we will see momentarily, it is this assumption – which is also the standard assumption used in the development accounting literature – that allows us to identify individual human capital stocks directly off individual wages. We discuss possible departures from this assumption later in section 4. In particular, Appendix A.6 provides a set of weaker assumptions under which we can no longer identify individual human capital stocks, but aggregate human capital stocks are still correctly identified.

$^{20}$An alternative formulation would have been to let human capital instead of skill prices depend on cohort quality: $h_{ict} = \exp(g(s_{ict}) + f(x_{ict}) + \psi_c), w_{ct} = \bar{w} \exp(\gamma_t)$. This would have some different implications for estimating human capital stocks. However, note that with this specification, it is no longer true that the human capital stock of an individual with $s = x = 0$ is one, a normalization we imposed above. We prefer to work with this normalization because it effectively defines human capital to be that part of wages that is only due to schooling or experience.
3.2 Baseline Results: No Cohort or Time Effects

To fully account for changes in the slope of the experience-earnings profile, we estimate the following equation separately for each country:

\[
\log y_{ict} = \alpha + \theta s_{ict} + \sum_{x=1}^{46} \phi_x D_{ict}^x + \varepsilon_{ict},
\]

where \( D_{ict}^x \) is a dummy variable for a worker’s number of years of experience. The coefficient \( \phi_x \) estimates the average wage of workers with \( x \) years of experience relative to the average earnings of workers with zero years of experience. In terms of our notation from the previous section, \( \phi_x = f(x) \). That is, the coefficient estimate corresponding to each experience level, \( x \), identifies the experience-earnings profile evaluated at that point \( x \). We first estimate this equation without including either cohort or time effects, \( \gamma_t = \psi_c = 0 \), but argue in the next subsection that results are broadly robust to including these.

The experience-earnings profiles for all countries are plotted in Figures 1 to 4, organized by quartile of the world income distribution in our data. In Figure 5 and future figures we focus on the six largest countries in our sample, the estimates for which are representative of the ones for our entire sample of countries. Figure 6 plots the same profiles with the corresponding 99% confidence intervals, showing that the profiles are very tightly estimated. Figure 7 presents the same information in yet another way and plots the height of each country’s experience-earnings profiles evaluated at twenty years of experience against per capita GDP. These estimates provide two main insights. First, they show that experience-earnings profiles in poor countries are typically flatter than those in rich countries.\(^{21}\) In particular, the initial increase for inexperienced workers is much steeper for rich countries. Second, the experience-earnings profiles are highly nonlinear.

To assess the degree of non-linearity, we experiment with a variety of polynomial specifications of the form

\(^{21}\)The profiles are not flatter in poor countries at each level of experience, point by point. Instead, the average slope in poor countries is less than in rich countries. That being said, we choose the word “flat” because we want to emphasize that these profiles are about changes in wages relative to zero years of experience (the intercept of the experience earnings profiles is set to zero for all countries). So our finding is definitely a statement about slopes rather than levels.
\[ \log y_{ict} = \alpha + \theta s_{ict} + \sum_{k=1}^{p} \phi_k x_{ikt} + \varepsilon_{ict}, \quad (6) \]

where the log wage of individual \( i \) living in cohort \( c \) during year \( t \) is a function of the years of schooling, \( s_{ict} \); and the years of experience, \( x_{ict} \). This is the special case of the model presented in the previous section with \( g(s) = \theta s \) and \( f(x) = \sum_{k=1}^{p} \phi_k x^k \). The literature typically uses a linear-quadratic Mincerian specification, that is the case \( p = 2 \). Can such a Mincerian specification accurately capture the shape of the experience-earnings profiles? To answer this question, Figure 8 graphically compares the Mincer specifications to the fully flexible experience-earnings profiles. Comparing with Figure 5, it can be seen that the standard linear-quadratic Mincerian specification, \( p = 2 \), does a poor job at capturing the shape of the experience-earnings profiles. If a second order polynomial cannot capture the shape, what is the lowest order polynomial that can? We find that a quintic, \( p = 5 \), provides an accurate but relatively parsimonious approximation to the fully flexible specification. Figure 9 presents the results with a quintic specification. It can be seen that, in contrast to the Mincer specification, such a quintic specification captures the shape of the fully flexible experience-earnings profiles in Figure 5 well. These differences are important because they illustrate why the quadratic Mincer equation is inappropriate for capturing the differences in returns to experience across countries.

There are two key points to take away from the results thus far. First, experience earnings profiles in poor countries tend to be flatter than those in rich countries. Second, Mincer estimates can be misleading and understate the difference in returns to experience between rich and poor countries because returns vary across experience levels in a way that cannot be captured with a linear-quadratic specification.

### 3.3 Controlling for Cohort and Time Effects

In this section, we will show that the estimates for returns to experience and aggregate human capital stocks can differ substantially for some countries when we control for cohort effects, but that our main finding – that experience-earnings profiles in poor countries tend to be flatter than their counterparts in rich countries – is robust to their inclusion.

Ideally, we would be able to identify the functions \( g(s_{ict}) \) and \( f(s_{ict}) \) with both cohort and time
effects and use them to compute our measure of human capital (1). However, with a fully flexible specification, it is impossible to simultaneously control for year and cohort effects due to the familiar problem that schooling, experience, year and birth year are collinear.\textsuperscript{22} Instead, we can estimate the returns to experience either controlling for \textit{i}) cohort fixed effects or \textit{ii}) time fixed effects. Since some of our countries have data for only every ten years, we construct birth-cohort categories of ten birth-year intervals. Our cohort fixed effects are therefore dummy variables that take the value of one if a worker belongs to a birth-cohort category. The estimated coefficients are plotted in Figures 10 and 11.

In Figure 10, we plot the estimates for equation (5) when year controls are included. There is little difference from the cross-sectional estimates shown in Figure 5. These results indicate that there are not too many changes over time, which is not altogether surprising since our data covers a relatively short time period (between ten and fifteen years). As with the cross-sectional estimates shown in Figure 5, these estimates show that the experience-earnings profiles are highly non-linear.

Figure 11 presents the coefficients for the returns to experience when birth-cohort fixed effects are included in equation (5). In contrast to the addition of time effects, adding cohort effects does change the experience-earnings profiles substantially for some countries. The influence of cohort controls is most prominent for China, where these controls cause the returns to dramatically increase and the experience-earnings profile to rotate counter-clockwise. The results indicate that cohorts may be improving over time so that not including cohort controls may bias the results downwards. For all countries except China, the inclusion of cohort controls does not alter the main finding of our benchmark exercise, namely that experience-earnings profiles in poor countries tend to lie those in rich countries.

We also repeat the same exercise using the quintic specification, (6) with $p = 5$. Results are shown in Figures 12 and 13. One potential advantage of a parametric specification such as this quintic is that – in contrast to the fully flexible estimates – it is now possible to simultaneously control for cohort and time effects. We do so and present results in Figure 14. Not surprisingly, the resulting experience-earnings profiles lie broadly between those with only time or cohort effects in Figures 12 and 13. Being able to simultaneously control for cohort and time effects is an attractive

\textsuperscript{22} In particular, schooling, experience, year and birth year are linked through the identity $t = \text{age}_{ict} + c = s_{ict} + x_{ict} + 6 + c$. 

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feature of a polynomial specification. But note that the relative contribution of each is identified entirely off the functional form. We therefore continue to report cross-sectional results, as well as those with either of time or cohort effects.

The estimates in this section show our results are at least qualitatively robust to the inclusion of cohort and time controls.

3.4 Returns to Experience Estimates from Earlier Studies

Before moving forward, we note that Bils and Klenow (1998, 2000) collected cross-sectional Mincer estimates from various studies for a large number of countries and found no relationship between the returns to experience and income. This naturally begs the question of why our estimates differ. One obvious difference between our study and the earlier studies (e.g. Psacharopoulos, 1994; and Krueger and Pischke, 1992) from which Bils and Klenow (1998, 2000) extract their estimates is the underlying data. However, we believe that the difference in data is not the sole cause in the difference in results and that the specification of the functional form of experience-earnings profiles also matters. To see this, Figure 15 plots the height of experience-earnings profiles at twenty years of experience that are obtained using a linear-quadratic Mincer specification against a country’s income level. There is only a weak relationship. It is only when we allow the estimates to vary flexibly across experience levels or use at least a quintic specification, that a pattern emerges. Hence, the difference in the data is important mostly in that the large samples allow us to conduct an estimation strategy that require larger samples. The fact that we allow for returns to experience to vary across experience levels according to a specification that is much more flexible than the simple Mincerian linear-quadratic one is key for our results.\textsuperscript{23}

4 Aggregate Human Capital and Development Accounting

Having estimated returns to experience across countries, we now calculate individual and aggregate human capital stocks that fully take into account cross-country differences in returns to experience.

\textsuperscript{23}Another natural concern is that we examine select countries that exhibit a correlation between returns to experience and income levels. To address this, we have examined the estimates that Bils and Klenow (1998, 2000) used for our countries (from Appendix B of their paper). We have calculated the implied returns to experience for workers with twenty years of experience and correlated them with income levels. There is no relationship. This alleviates the concern that our main result that returns to experience is higher for higher countries is an artifact of our choice of countries.
We then conduct a standard development accounting exercise with our improved measures of aggregate human capital stocks, and show that taking into account cross-country differences in returns to experience substantially increases the fraction of cross-country income differences that can be explained by human and physical capital stocks.

4.1 Human Capital from Experience

We find it useful to decompose individual human capital stocks into the components due to experience and schooling: $h_{it} = h_{it}^S h_{it}^X$ where

$$h_{it}^X = \exp(f(x_{it})), \quad h_{it}^S = \exp(g(s_{it})).$$

Analogously, the part of aggregate human capital due to experience only is just the average of the individual stocks across individuals and over time

$$H^X = \frac{1}{T} \sum_{t=1}^{T} \frac{1}{N_t} \sum_{i=1}^{N_t} h_{it}^X.$$

Our estimates for aggregate human capital stocks due to experience therefore simply integrate under the experience-earnings profiles estimated in the previous section, using the distribution of work experience from the data. The distributions of work experience in our six representative countries are displayed in Figure 16 which shows that there are some cross-country differences in experience distributions, but these are hardly dramatic.\(^{24}\) Similarly, Figure 17 plots the average potential experience for all countries in our sample against GDP per capita. There is not much variation in average experience across countries and certainly no clear difference between rich and poor countries.

Figures 18-21 plot the implied human capital from experience against per capita GDP for each

\(^{24}\) The experience histogram for India is much more “spikey” than that for the other five countries. This comes from the age heaping phenomenon: some people tend not to give their exact age in a survey, instead, they round their age up or down to the nearest number that ends in 0 or 5. This phenomenon which is also observed in other countries, especially in Bangladesh, is a potential issue because age heaping creates measurement error in reported ages and as such may generate downward bias in experience-earnings profiles. We examine this issue in detail in Appendix A.4 and argue that even extreme age heaping is able to quantitatively generate the large differences in experience-earnings profiles we observe across countries.
country during this period. We obtain GDP data from the World Bank Development Indicators. GDP is measured in constant 2000 USD. For China, we calculate urban per capita GDP by normalizing national GDP by the ratio of urban-to-rural real wages, which we obtain from the *China Statistical Yearbooks*. For these Figures, the human capital stocks from experience are calculated using the quintic specification (6) and each Figure corresponds to a different combination for the inclusion of cohort and year controls. These figures show clearly that there is a strong positive relationship between human capital from experience and income levels. The estimates of experience human capital stocks for each country that were used in the figures are reported in Table A.1 in the Appendix.\textsuperscript{25}

For comparison, Figure 22 plots the implied human capital from experience based on the Mincer estimates of returns to experience and plot these estimates against income levels (the results are similar when we control for either cohort or year effects or both). The y-axes of these figures cover the same range as the y-axes in figures 18-20. Consistent with our earlier findings that Mincer estimates of returns to experience cannot fully capture the shape of the experience-earnings profiles, a comparison of the two sets of figures show that the relationship between human capital from experience and income levels is much weaker when using Mincerian returns to experience.

We identify individual human capital stocks directly off individual wages and then aggregate these to obtain aggregate human capital stocks. This is possible because of our assumption that individuals are paid their marginal products of labor. While this is the standard assumption used in the development accounting literature, it is admittedly a strong one. We therefore find it useful to briefly point out that there is a set of weaker assumptions under which it is still possible to identify aggregate human capital stocks even though it is no longer possible to identify their individual-specific counterparts. These are spelled out in more detail in Appendix A.6. The idea is that we can allow individual wages to equal marginal products plus zero-sum transfers, either across individuals or over the lifecycle of a given individual.\textsuperscript{26}

\textsuperscript{25}Columns (1)-(3) are based on the fully flexible returns to experience estimates from equation (5) for when we use cross-sectional estimates of returns to experience, include cohort controls, and alternatively, year controls. Columns (4)-(7) are based on the quintic specification from equation (6), for the cases of neither cohort or year controls, time controls only and cohort and time controls (the latter of which is possible with a quintic in contrast to the flexible specification).

\textsuperscript{26}As explained in more detail in Appendix A.6, zero-sum transfers across a given individual additionally require the assumption that the economy has a stationary age distribution and stationary schooling choices in which case they are isomorphic to transfers across individuals.
4.2 Total Human Capital Stocks Due to Both Schooling and Experience

We define the total human capital stock (due to both schooling and experience) in a country to be the average of individual human capital stocks, \( h_{it} = \exp(g(s_{it}) + f(x_{it})) \),

\[
H = \frac{1}{T} \sum_{t=1}^{T} \frac{1}{N_t} \sum_{i=1}^{N_t} h_{it}.
\] (7)

Our estimates of these human capital stocks are based on our estimated returns to schooling and experience from our quintic specification.

Table 1 summarizes our country-specific estimates of aggregate human capital stocks and presents two measures of the cross-country dispersion of total human capital stocks. The first measure we use is the log variation in human capital stocks, and the second measure is the ratio of the human capital stock of the country at the 90th percentile in the income distribution to that at the 10th percentile. The 90th percentile country has an experience human capital stock that is 1.97 times as large as that of the 10th percentile. For sake of comparison, we also present the difference in the schooling human capital stock which equals 1.90. This is the measure traditionally used by the literature. These results show that cross-country differences in human capital due to experience (as measured by the 90-10 ratio) are roughly as big as those due to schooling. If dispersion is instead measured using the log variation in column one, dispersion is somewhat larger for schooling than for experience human capital stocks, but both are of the same order of magnitude. Finally, the third row of the Table reports the dispersion of total human capital stocks, that is taking into account cross-country differences in returns to experience (going from row one to three) roughly doubles the dispersion in human capital across countries. Table 2 shows that these numbers change somewhat when we include cohort and year effects, but that our main finding – that allowing returns to experience to vary across countries increases cross-country human capital gaps – is robust to their inclusion.

Tables 1 and 2 are based on more detailed country-specific estimates that are reported in A.2 in the Appendix (and also those in Table A.1). Column (1) reports each country’s human capital stock (relative to the United States), taking into account only schooling, but not experience, corresponding to row one in Table 1. Columns (2)-(5) take into account both schooling and experience, corresponding to row three in Table 1. It can be seen that taking into cross-country differences in
experience-earnings profiles decreases relative human capital stocks in poor countries. For example, the total human capital stock of India falls from roughly 60 percent of the US to around 25-30 percent (with some variation in this number depending on the estimation strategy).

For comparison with the existing development accounting literature, A.2 also reports some numbers that are more directly comparable to those in Caselli (2005). In his benchmark exercise, Caselli takes into account cross-country differences in schooling but not experience or returns to experience. Since adding experience to the accounting exercise is the main contribution of this paper, we use his estimates that do not take into account experience as the benchmark against which to evaluate our measures of the human capital stock. To this end, we now assume that the part of a country’s total human capital stock that is due to schooling only is given by Caselli’s estimates, and denote them by $H^S$. These estimates are reported in column (6) of the Table. We then calculate our measure of aggregate human capital as the product of Caselli’s estimates and our aggregate human capital stocks due to experience from Table A.1, $H = H^S H^X$. This way of calculating aggregate human capital stocks clearly isolates the contribution of returns to experience vis-à-vis the existing literature. Table A.2, columns (7) to (10), report the resulting aggregate human capital stocks relative to the United States. While Caselli’s schooling human capital stocks in column (6) are different from our own estimates in column (1), our main finding remains, namely that taking into account experience tends to decrease human capital stocks in poor countries relative to the US.

### 4.3 Development Accounting

To facilitate comparison with the existing development accounting literature, we use the same accounting method as in the review by Caselli (2005). That is, our accounting procedure uses a Cobb-Douglas aggregate production function $Y = K^\alpha (AH)^{1-\alpha}$ where $Y$ is a country’s real GDP, $K$

Note that this way of computing total human capital stocks is consistent with the definition in equation (7) only under the assumption that all individuals in a country have the same years of schooling $s_{it} = \bar{s}$ so that

$$H = \frac{1}{T} \sum_{t=1}^{T} \frac{1}{N_t} \sum_{i=1}^{N_t} \exp(g(\bar{s}) + f(x_{it})) = \exp(g(\bar{s})) \frac{1}{T} \sum_{t=1}^{T} \frac{1}{N_t} \sum_{i=1}^{N_t} \exp(f(x_{it})) = H^S H^X$$

and that the function $g(s)$ is the same as the one used by Caselli. If instead, years of schooling vary within the population as in our sample, this decomposition is invalid. Our estimates of the total human capital stocks in columns (4)-(7) instead take into account within-country variation in years of schooling.

We here report only the results from the quintic specification but note that the results for the fully flexible specification are broadly similar (except, of course for the case of both cohort and time effects which cannot be computed in that case).

\[17\]
its physical capital stock, $A$ is total factor productivity and $H$ is our measure of the human capital stock. The capital share is assumed to equal $\alpha = 1/3$.

We follow Caselli and calculate his measures “Success-1” and “Success-2” that are designed to measure the fraction of cross-country income differences explained by factors of production only

$$success_1 = \frac{\text{var}(\ln Y_{KH})}{\text{var}(\ln Y)}$$

$$success_2 = \frac{Y_{90}^{KH}/Y_{10}^{KH}}{Y_{90}/Y_{10}}$$

where $Y_{KH} = K^{\alpha}H^{1-\alpha}$ is the component of output explained by factors of production. That is, “Success-1” is the fraction of the variance of log GDP per capita that is explained by human and physical capital. Similarly, “Success-2” is the fraction of 90th-to-10th percentile ratio of GDP that is explained by these factors of production. Table 3 presents these measures of success for different measures of the aggregate human capital stocks. When human capital is identified with schooling only as in most of the literature, the two measures of success are between forty and fifty percent. In Table 1 we showed that cross-country differences in experience human capital are roughly as big as those in schooling human capital. This is now also reflected in Table 3: comparing the first and the second row reveals that schooling and experience human capital are roughly equally important determinants of cross-country income differences. Finally, when both schooling and experience are taken into account as in the third row, both measures of success increase dramatically. Physical and human capital taken together now account for roughly two thirds of the variation in cross-country income differences as compared to less than half when experience is not taken into account.

We have also conducted our development accounting exercise on a country-by-country level. To do this we report a slightly modified version of “Success-2”

$$success_2 = \frac{Y_{US}^{KH}/Y_{Poor}^{KH}}{Y_{US}/Y_{Poor}}$$

That is, we report the fraction of the GDP gap between the US and one of our “poor” countries (all other countries in our sample) that can be explained by factors of production only. Table A.3 in the Appendix first reports Caselli’s numbers for output and physical capital in columns (1) and (2). The estimates for “Success-2” are presented in rows (3)-(12). We first report results for the
case where we use our own estimates of schooling and total human capital stocks in columns (3)-(7). The results indicate that taking into account cross-country differences in returns to experience when calculating aggregate human capital stocks allows one to account for a substantially larger fraction of cross-country income differences than the existing literature. In columns (8)-(12), we then report results for the case where we used Caselli’s human capital stocks due to schooling as a basis to calculate total human capital stocks. These estimates are the most directly comparable to the literature. Again, taking into account cross-country differences in experience-earnings profiles substantially increases the “Success-2” measures.

Note that for some countries (for example, Australia, Chile, Italy and Mexico) and under some specifications, our estimates even modestly over-predict the GDP gap to the United States when taking experience into account. That is, we estimate that human capital in these countries is so low relative to the US, that the only way of accounting for the observed GDP gap is for these countries to have higher TFP than the US.

5 Potential Mechanisms

In this paper, we have first documented a new fact, namely that experience-earnings profiles in poor countries are flatter than in rich countries. We have then argued that taking this fact into account in a standard development accounting exercise greatly increases the contribution of physical and human capital to cross-country income differences. But why are experience-earnings profiles in poor countries flatter than in rich countries? The purpose of this section is to explore some potential explanations for this fact. We first ask to what extent our finding can be explained by cross-country differences in the composition of the labor force across, say, sectors and occupations. But we find that such composition effects explain only a relatively small fraction of cross-country differences in returns to experience and experience human capital stocks. In light of this finding, we then discuss some existing theories that can potentially explain our empirical finding, among them that low total factor productivity in poor countries may lead to low human capital accumulation and flat experience-earnings profiles.
5.1 Composition Effects

It is well known that even within developed countries such as the United States, some groups of individuals have steeper experience-earnings profiles than others. For example, Herrendorf and Schoellman (2011, Figure 4b) document that the experience-earnings profile is flatter in agriculture than in non-agriculture. And college graduates have steeper age-earnings profiles than high school graduates (Carroll and Summers, 1991, Figures 10.7a and 10.8a; Guvenen, 2007, Figure 2; Kam-bourov and Manovskii, 2009, Figures 3, 6, 8 and 10) suggesting that also experience-earnings profiles may differ (Elsby and Shapiro, 2012, Figure 3). This raises the possibility that the cross-country differences in returns to experience that we have documented are partly or even mostly due to composition effect: experience-earnings profiles within, say, agriculture and non-agriculture could be the same in all countries, but poor countries simply have more workers in agriculture and hence the average experience-earnings profiles in poor countries are flatter than those in rich countries.

The purpose of this subsection is to investigate this possibility, thereby shedding some light on the potential mechanism that drives cross-country differences in returns to experience. We will show that composition effects do not seem to be an important driver of these differences, at least at the level of disaggregation that we are able to pursue with our data.

Agriculture. Let us begin with composition effects across sectors, in particular agriculture versus non-agriculture. Composition effects along other dimensions can be analyzed in a similar fashion. It is well known that poorer countries employ a higher share of their labor force in agriculture. Figure 23 shows that this fact also holds in our sample. Differences in sectoral composition across countries therefore have the potential to explain a large portion of the variation across countries in returns to experience and experience human capital stocks. We extend our simple model in section 3.1 to allow for differences in human capital accumulation across sectors. In particular, we now allow the human capital production functions (1) to be different for agriculture (sector $A$) and

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30 At some level of course and almost tautologically, all cross-country differences in experience human capital stocks must be due to composition effects. That is, the more fine-grained our decomposition, the bigger should be the fraction of dispersion in human capital stocks that can be explained by composition effects.

31 Agricultural employment shares – like all other employment shares used in this section – are computed using individual survey weights rather than simply counting people in our sample.
non-agriculture (sector $N$). Human capital of individual $i$ at time $t$ in country $j$ is now

$$h_{itj} = \exp(g_j(s_{itj}; D_{itj}) + f_j(x_{itj}; D_{itj})) \quad \text{where} \quad D_{itj} \in \{A, N\}$$

is the sector that individual $i$ is active in at time $t$. Similarly to before, the functions $f_j(\cdot; A)$ and $f_j(\cdot; N)$ can be identified off the experience-earnings profiles for agriculture and non-agriculture. That is, we simply estimate equation (4) separately for each of the two sectors. Having done so we construct aggregate human capital from experience in sector $D \in \{A, N\}$ and country $j$ as

$$H^X_{D,j} = \frac{1}{T} \sum_{t=1}^{T} \frac{1}{N_{Dt,j}} \sum_{i:D_{itj}=D} \exp(f_j(x_{itj}; D), \quad (8)$$

where $N_{Dt,j}$ is the number of individuals in country $j$ at time $t$ that are employed in sector $D \in \{A, N\}$. Aggregate experience human capital in country $j$ is then simply a weighted average of the sectoral experience human capital stocks

$$H^X_j = \ell_{A,j} H^X_{A,j} + (1 - \ell_{A,j}) H^X_{N,j},$$

where $\ell_{A,j}$ is the employment share in agriculture in country $j$. Figure 24 shows the height of the experience earnings profile at twenty years of experience in agriculture plotted against that in non-agriculture. It can be seen that all countries except Italy are represented by a point below the 45 degree line; that is, for all countries except Italy the experience-earnings profiles in agriculture are flatter than those in non-agriculture. The finding documented by Herrendorf and Schoellman (2011, Figure 4b) for the US therefore also holds for most other countries in our sample. Since experience human capital stocks are closely related to the height of the experience-earnings profiles, a plot of these across sectors looks almost identical to Figure 24 (with rescaled axes) and so we omit the Figure to save space.

To assess the importance of agriculture versus non-agriculture composition effects, we conduct a counterfactual exercise. We ask: what would a country’s experience human capital be if that country had the US employment share in agriculture? That is, we compute the following counterfactual
experience human capital stock for each country \( j \)

\[
\tilde{H}^X_j = \ell_{A,US} H^X_{A,j} + (1 - \ell_{A,US}) H^X_{N,j}.
\]

If all of cross-country differences in experience human capital stocks were due to sectoral differences, then this counterfactual would eliminate all such differences. Figure 25 graphs the counterfactual human capital stocks (those using the US agricultural employment shares) against the actual human capital stocks. If composition effects explained all of cross-country differences in experience human capital stocks, all countries would lie on a straight horizontal line at the level of the US human capital stock. But instead all countries lie very close to the 45 degree line. That is, counterfactual human capital stocks are very similar to the actual ones and therefore composition effects do not seem to be important in practice.

To make this point more rigorously, we decompose our measures of cross-country dispersion of experience human capital stocks from Table 1 as follows

\[
\frac{H^X_{90}}{H^X_{10}} - 1 = \left[ \frac{H^X_{90}}{H^X_{10}} - \frac{\tilde{H}^X_{90}}{\tilde{H}^X_{10}} \right] + \left[ \frac{\tilde{H}^X_{90}}{\tilde{H}^X_{10}} - 1 \right] \quad \text{part due to composition effects} \quad \text{part due to within-sector diff’s in } H^X
\]

and similarly for our other dispersion measure, the variance of the logarithm of experience human capital stocks. Table 5 reports the results of this decomposition. Agriculture versus non-agriculture explain 5-7 percent, that is a relatively small fraction, of the log variation in experience human capital stocks across countries.

**Schooling.** Another obvious contender for driving cross-country differences in returns to experience are experience-earnings profiles that differ with educational attainment. This is because educational attainment is lower in poor countries, a fact we confirm for our sample in Figure 26 which plots the share of workers with less than ten years of schooling against per capita GDP. To allow for the possibility that different levels of schooling are associated with different returns to experience, we again generalize the human capital production function (1). In particular, we now allow for a functional form that is non-separable in schooling and experience, \( h_{it} = \exp(m(s_{it}, x_{it})) \).
If the function $m$ has a positive cross-derivative, schooling and experience are complements, capturing the idea that one has to “learn (in school) how to learn (on the job).” If instead $m$ has a negative cross-derivative, this would suggest that schooling and experience are substitutes.

We work with a simple cutoff specification that allows for different returns to experience according to whether a worker has “high” ($H$) or “low” ($L$) years of schooling, that is whether his years of schooling are larger or smaller than some cutoff $\bar{s}$ that is common across countries

$$h_{itj} = \exp(g_j(s_{itj}; D_{itj}) + f_j(x_{itj}; D_{itj})) \quad \text{where} \quad D_{itj} = \begin{cases} L, & s_{itj} \leq \bar{s} \\ H, & s_{itj} > \bar{s} \end{cases}.$$ 

In practice we use the threshold $\bar{s} = 10$. The aggregate experience human capital stock for a given schooling categories is then again simply the average human capital across all individuals in that category, that is calculated as in equation (8). Figure 27 shows the height of the experience-earnings profile for workers with low schooling (less than ten years) plotted against the height for those with high schooling (more than ten years). Somewhat surprisingly, in many countries including the US, workers with low educational attainment have steeper experience-earnings profiles than their educated counterparts. This is in contrast to the opposite pattern that is typically found for age-earnings profiles (Carroll and Summers, 1991; Guvenen, 2007; Kambourov and Manovskii, 2009) and that we also document for our dataset. Appendix A.7 discusses in more detail the reasons why experience-earnings profiles are steeper for workers with low schooling even though their age-earnings profiles are steeper. As was the case in the agriculture decomposition, a plot of experience human capital stocks across sectors is basically just a rescaled version of the plot of the height of experience-earnings profiles, Figure 27, and so we omit it to save space.

We next conduct a similar counterfactual exercise as above, that is we compute the implied experience human capital stocks if all countries had the share of highly educated individuals of the US. Figure 28 plots these counterfactual human capital stocks against the actual ones. Many countries actually lie below the 45 degree line, suggesting that the cross-country gaps in the counterfactual human capital stocks are even larger than those in the the actual human capital stocks. Table 6 reports the fraction of cross-country dispersion in experience human capital stocks that is due to schooling composition effects. The number is negative, suggesting that schooling composition effects
cannot explain the cross-country differences in returns to experience and experience human capital stocks.

**Other Composition Effects** Using the same approach as for the agriculture versus non-agriculture and the schooling decompositions, we have probed the data for composition effects along other dimensions. We tried: “Services vs. Non-Services”, “Manufacturing vs. Non-Manufacturing”, “Government vs. Private Sector Employees”, “Males vs. Females”, “Urban vs. Rural”, and “Full Time vs. Part Time.” We also tried a “Agriculture-Manufacturing-Services-Government” decomposition in which we computed counterfactual human capital stocks with all sectoral shares set equal to their US values simultaneously. In results that are not reported here but available upon request, we found that none of these decompositions go very far in explaining cross-country differences in returns to experience. In particular, for all of them the fraction of cross-country dispersion in experience human capital explained by composition effects is less than the 5-7 percent explained by the agriculture versus non-agriculture composition reported in Table 5.

5.2 Low TFP as a Driver of Flat Experience-Earnings Profiles

If composition effects do not explain why experience-earnings profiles in poor countries are flatter than in rich countries, what does? Some existing theories do in fact predict exactly this pattern, among them a recent one by Manuelli and Seshadri (2010). In their model, it is low total factor productivity in poor countries that leads to little human capital accumulation and flat experience-earnings profiles. They consider a Ben-Porath type model in which human capital accumulation requires both time and nontime inputs (i.e. goods inputs, for example books, equipment, or buildings). Low TFP means that the price of nontime inputs relative to the wage per unit of human capital is high. This implies that individuals purchase fewer of these nontime inputs and accumulate less human capital, both within school and on the job. A symptom of this are flat experience-earnings profiles.

More generally, we envision an entire class of theories that is consistent with our empirical fact that experience-earnings profiles in poor countries are flatter than in rich countries. The common feature of this class of theories is simply that TFP and experience human capital accumulation are complementary, meaning that an increase in TFP raises the returns to the accumulation of
experience human capital.

5.3 Other Explanations for Flat Experience-Earnings Profiles

Yet another class of theories (Lucas, 2009; Lucas and Moll, 2011; Perla and Tonetti, 2011) posits that an important determinant of an individual’s learning over the lifecycle is the “learning environment” the individual finds himself in. That is, human capital is accumulated through social interactions with others: if an individual with little knowledge gets the chance to interact with someone more knowledgeable, he can learn from the latter. Through this process, the individual accumulates knowledge over the lifecycle. And more or better interactions lead to steeper age-earnings profiles. All determinants of the frequency or quality of such social interactions are therefore also potential determinants of cross-country differences in returns to experience. One example is the quality of “communication technology.” Another example is the degree of urbanization within a country: if social interactions are easier and more frequent in cities and harder in remote rural areas, then a large rural population share would lead to flat experience-earnings profiles.

6 Conclusion

Why are some countries so much richer than others? What fraction of cross-country income differences can we account for with data on observable factors of production only? We have addressed these questions by conducting a development accounting exercise that adds to previous exercises by allowing the returns to worker experience to vary across countries. By using recently available large sample micro data from 28 countries from very different parts of the world income distribution, we have documented a new fact, namely that experience-earnings profiles in poor countries are flatter than in rich ones. We have then argued that taking this fact into account in a standard development accounting exercise, roughly doubles the dispersion in human capital across countries and increases the contribution of physical and human capital to cross-country income differences from less than one half to roughly two thirds.

While it is beyond the scope of the paper, explaining the mechanism that causes experience-earnings profiles to be lower in developing countries is certainly an important avenue for future research. As already discussed, one possible explanation is that TFP differences themselves lead to
the differences in experience profiles (for example, through a mechanism such as the one outlined by Manuelli and Seshadri, 2010). Another possibility is that workers in developing countries may have less incentive to accumulate human capital if governments in developing countries tax wages more than in richer countries. Guvenen et al. (2011) show that a version of this mechanism explains a substantial fraction of Europe-U.S. differences in wage inequality and life-cycle wage growth. Or it could be that there is simply more scope for learning over the life cycle in advanced economies, due to e.g. relatively more use of advanced technologies, or greater opportunities for improving one’s productivity outside of the workplace. Finally, a recent study by Hsieh and Klenow (2011) documents a fact that is in some ways the flip-side of our fact for firms: that manufacturing plants grow much less over the lifecycle in India and Mexico than in the United States. An interesting question is whether these two facts are linked in any way or perhaps even driven by the same factor.\footnote{For one, Seshadri and Roys (2012) propose a theory that can simultaneously explain both facts.}
References


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A Appendix

A.1 Data and Variables

The surveys we employ in our analysis are listed below for each country. All surveys are nationally representative unless noted. We obtained a number of surveys from the Food and Agriculture Organization’s (FAO) Rural Income Generating Activity (RIGA) database; these surveys are available here: www.fao.org/economic/riga/riga-database/en/. We obtained a number of other surveys through the Integrated Public Use Microdata Series (IPUMS) (Minnesota Population Center, 2011; King et al., 2010), which can be found here: www.ipums.org. The remaining surveys were made available to us by the statistical agencies of the countries in question or other sources, as listed below.


- Brazil: *Recenseamento Geral do Brazil, Censo Demográfico*, 1970 (5% sample), 1980 (5% sample), 1991 (5.8% sample), and 2000 (6% sample), from the Instituto Brasileiro de Geografia e Estatística (IBGE), available from IPUMS, and *Pesquisa Nacional por Amostra de Domicílios*, yearly from 2001 to 2010, from IBGE.

- Canada: *Census of Canada*, 1971 (1% Sample), 1981 (2% Sample), 1991 (3% Sample) and 2001 (2.7% Sample), available from IPUMS.

- Chile: *National Socioeconomic Characterization Survey (CASEN)*, 2000 and 2009, from the Chilean Ministry of Planning and Cooperation.

- China: *Urban Household Surveys* (0.01% of urban households, 27 cities), year from 1989 to 2005; *representative of urban areas*.


• Germany: *German Socioeconomic Panel (SOEP)*, yearly from 1991 to 2009, from the German Institute for Economic Research (DIW Berlin).

• India: *Socio Economic Survey* by National Sample Survey Organization, 1993 (0.07% of households), 1999 (0.07% of households), 2004 (0.06% of households), available from IPUMS.


• Mexico: *XI General Population and Housing Census*, 1990 (10% sample); *Population and Dwelling Count*, 1995 (0.4% of sample); *XII General Population and Housing Census*, 2000 (10.6% of sample), available from IPUMS.


• Panama: *Censo Nacional de Población y de Vivienda de Panamá*, 1990 (10% sample), available from IPUMS, and the *Encuesta de Condiciones de Vida*, 2003, from the Dirección de Estadística y Censos de Panamá, available from the FAO RIGA database.

• Peru: *Encuesta Nacional de Hogares*, 2004 and 2010, from the from the Instituto Nacional de Estadística y Informática.

• Puerto Rico: *Census of Population and Housing*, 1970 (1% Sample), 1980 (5% Sample), 1990 (5% Sample), 2000 (5% Sample); *American Community Survey*, 2005 (1% Sample), available from IPUMS.

• Russia: *Russia Longitudinal Monitoring Survey*, yearly from 2000 to 2010, available from the Carolina Population Center at the University of North Carolina, Chapel Hill.

• South Korea: *Korea Labor and Income Panel Study*, yearly from 1999 to 2008, from the Korea Labor Institute, available from the Cornell Department of Policy Analysis and Management.


• United Kingdom: *British Household Panel Survey*, yearly from 1992 to 2009, from the Institute for Social & Economic Research at the University of Essex.

• United States: *Census of Population and Housing*, 1960 (1% Sample), 1970 (1% Sample), 1980 (5% Sample), 1990 (5% Sample), 2000 (5% Sample); *American Community Survey*, 2005 (1% Sample); *Current Population Survey*, yearly from 1980 to 2010; all available from IPUMS.


All calculations in our analysis are weighted using the applicable sample weights for each survey. We express all earnings and wage data in local currency units of the most recent year in the data using the consumer price index of the country in question, taken from the IMF’s International Financial Statistics database. In each survey we drop the top and bottom 1% of earners to remove potential outliers, and to minimize the impact of potential cross-country differences in top-coding procedures.

For most countries, we measure hours as the actual hours worked in the past week (or in some recent reference week.) For the United States, Brazil (the census data), Italy and Puerto Rico, we measure hours as the usual weekly hours worked (which is what is available). For China, India, Panama (the census data), Taiwan and Thailand, we have no hours data available, and impute hours as the average hours worked in all other countries for the individual’s level of experience.

For most countries, labor earnings and hours worked are for both primary and secondary jobs. In Argentina, Chile, France, South Korea and Uruguay, labor earnings and hours worked are for just the primary job. For Brazil (the census data) and Switzerland, we measure labor income as the total income earned of individuals reporting to be primarily wage earners (as opposed to self employed.) In most countries, earnings are reported at the monthly frequency. The exceptions are Australia, Canada, Germany, Jamaica, South Korea, and the United States, in which earnings are measured at the annual frequency, and India, in which earnings are measured at the weekly frequency. In all surveys, earnings are before taxes.

### A.2 Alternative Sample Restrictions

In the main analysis of the paper we chose to not restrict the population sample. In the spirit of development accounting, our aim was for our sample to be as nationally representative as possible, and for results to be consistent across, say, genders and industries. We showed that this is indeed the case. However, other studies often focus on a more restricted sample of the population, namely on males working full time in the private sector. So we here want to show the robustness of our
results to this sample restriction. Table A.4 reports the analogues of Tables 1 and 3, that is the dispersion across countries in human capital from schooling and experience as well as the two success measures, but for the sample restricted to only males. Table A.5 reports the same information but for only males working full time in the private sector. Relative to our benchmark case, including only male private full-time workers increases the dispersion of experience human capital rises, and decreases that of education human capital slightly. The net effect is an increase in explanatory power: “Success-1” is 0.78 for the restricted sample as compared to 0.66 in our benchmark exercise. We have also calculated our results for males working in the private sector but including both full and part time workers. These are not reported here but, not too surprisingly, the numbers are in between those in Table A.4 and Table A.5.

A.3 Alternative Definitions of Potential Experience

In our benchmark definition of potential experience is $\text{exper} = \text{age} - \text{schooling} - 6$ for individuals with 8 or more years of schooling completed, and $\text{exper} = \text{age} - 14$ if the individual has fewer than 8 years of schooling. This definition implies that individuals begin working at age 14 or when they finish school, whichever comes later. Figure A.1 provides our rationale for this definition. The figure plots the average fraction of individuals with positive earnings by age across all countries in our sample. For individuals aged 6, virtually none have positive wage earnings. As age rises, the fraction of individuals with positive earnings rises. At age 14 the fraction of individuals with any earnings begins to rise sharply, and by 18 on average a quarter of all people in our set of countries report having positive earnings.

We find a very similar patterns when looking across individual countries as well. In every country we observe a negligible fraction of 6 year olds with positive earnings; the maximum is Vietnam, at 1%. By age 11, there is still a negligible fraction of people with positive earnings in every country. The maximum are Ecuador and Bangladesh, at 5% and 4% respectively. By age 14, workers in many countries seem to show evidence of increases in wage employment. The average participation in wage earnings across countries rises to 5%. The maximum is reached in Ecuador with 22%. Thus, age 14 seems like a reasonable choice for the age individuals (not still in school) begin to accumulate experience, and hence our benchmark definition.

In what follows, we ask how our results change under two alternative definitions of potential experience. In the first, we assume that $\text{experience} = \text{age} - \text{schooling} - 6$ for all individuals. This definition implies that all individuals start work at age 6 or whenever they finish schooling, whichever comes later. This definition has been used extensively in Mincer regression analysis, particularly of richer countries such as the United States. One limitation of this definition when applied to poorer countries is that, as we document above, few individuals are actually working at age 6. Thus, this definition might be a poor approximation of potential experience.

Table A.7 reports, for this first alternative definition of potential experience, the dispersion across countries in human capital from schooling and experience, as well as the two success mea-
sures. Relative to the benchmark analysis, which assumes people start work at age 14, there is less
dispersion in human capital from experience across countries under the first alternative measure.
The variance in log human capital from experience is 0.093, and the 90-10 ratio is a factor 1.70. The
success measures for human and physical capital rise from 0.37 and 0.43 with only human capital
from schooling included to 0.52 and 0.62 with human capital from both schooling and experience.
Thus, while the combined importance of factors is somewhat lower than in the benchmark experi-
ment, it is still true that adding human capital from experience greatly increases the explanatory
power of human capital and physical capital in explaining income differences, and still takes the
importance of these factors above the 0.50 mark.

Table A.7 reports the dispersion across countries in human capital from schooling and experience,
as well as the two success measures, for the previous definition of experience, but under a sample
restriction that moves our exercise closer to previous work. Once we restrict the sample to include
only males working full time in the private sectors, the success measures rise respectively to 0.66
and 0.61, thus approaching the results obtained with the benchmark definition of experience.

In the second alternative definition of potential experience, we assume that experience = age
- schooling - 6 for individuals with 9 or more years of schooling, and experience = age - 15 for
individuals with fewer than 9 years of schooling. This definition implies that individuals begin
working at age 15 or after they finish school, whichever comes later. Given Figure A.1, this is
another plausible choice.

Table A.7 reports the same measures for the second alternative definition of potential experience.
Relative to the benchmark analysis, there is more dispersion in human capital from experience across
countries. The variance of log human capital across countries is now 0.118, and the 90-10 ratio is
now 2.01. The success measures for human and physical capital rise from 0.38 and 0.45 with only
human capital from schooling included to 0.68 and 0.71 with human capital from both schooling
and experience. Under this second alternative definition of human capital from experience, human
and physical capital now explain more than two thirds of income differences in the data, or far
higher than in the original Caselli (2005) exercises.

A.4 Measurement Error in Reported Age

We observed in our data that, more evidently in Bangladesh and India, the age distributions have
small heaps over the ages ending in 0 and 5. This is due to the age heaping phenomenon: some
people tend not to give their exact age in a survey, instead, they round their age up or down to the
nearest number that ends in 0 or 5. Age heaping creates measurement error in reported ages and as
such may generate downward bias in experience-earnings profiles. A major concern would therefore
be that the observed flatter profiles in developing countries may be the result of larger measurement
error due to stronger age heaping in low income countries. We here argue that even extreme age
heaping is not able to quantitatively generate the large differences that we observe across countries.
In order to do so, we take the United States data and artificially create measurement error in
reported age by randomly assigning a percentage of the sample to have not their actual age, but
their age reported to the nearest 5 years interval. We then re-estimate the United States’ profiles
and compare them to the benchmark case with no measurement error. In Figure A.2 we show the
experience-earnings profiles of United States for different values of artificially assigned age heaping.
We show how increasing the fraction of the sample to which we assign age reported to nearest 5
years interval does bias downward the profiles, but the effect is not quantitatively large. Even in
the extreme case in which we allow 90% of the United States population to report their age in 5
years intervals, still the resulting profile is very similar to the unbiased one and very far from the
profile of India.

A.5 United States Experience-Earning Profiles from CPS Data

We estimate the United States experience-earnings profiles using *Census of Population and Housing*
data available from IPUMS. However for the United States other data sources are available. In par-
ticular the most obvious alternative to Census data is the Current Population Survey. Reassuringly
the results obtained from the two different data sources are almost identical, as can be seen in
Figure A.3, where we show that the experience-earnings profiles estimated from Census data and
from CPS data lie exactly on top of each other.

A.6 An Acceptable Violation of the Wage Equals Marginal Product Assump-
tion

In the main text, we have identified individual human capital stocks directly off individual wages
and then aggregated these to obtain aggregate human capital stocks. This was possible because
of our assumption that individuals are paid their marginal products of labor. The purpose of this
Appendix is to argue that that there is a set of weaker assumptions under which it is still possible
to identify aggregate human capital stocks even though it is no longer possible to identify their
individual-specific counterparts. The idea is that we can allow individual wages to equal marginal
products plus zero-sum transfers, either across individuals or over the lifecycle of a given individual.

For sake of generality assume that human capital is produced using a functional form that is
non-separable in schooling and experience

\[
h_{ict} = \exp(m(s_{ict}, x_{ict})).
\]  

(9)

The formulation in section 3.1 is the special case \(m(s, x) = g(s) + f(x)\). In contrast to section 3.1,
we now depart from the assumption that individuals are paid their marginal products in efficiency
units of human capital. In contrast to equation (4), an individual’s hourly wage is now his human
capital times a skill price \(w_{ct} = \bar{w} \exp(\gamma_t + \psi_c)\) but plus a term \(B_{ict}\) that captures deviations from
the wage equals marginal product assumption:

\[ y_{ict} = (w_{ict}h_{ict} + B_{ict}) \exp(\varepsilon_{ict}). \]  

(10)

The formulation in section 3.1 is of course the special case in which \( B_{ict} \) equals zero identically. A potential identification problem arises if this transfer depends on schooling and/or experience. We therefore assume that

\[ B_{ict} = B(s_{ict}, x_{ict}) \exp(\gamma_t + \psi_c). \]  

(11)

The following Lemma shows that this departure from the assumption that wage equals marginal product may not be an issue as long as one is only concerned with correctly identifying aggregate human capital stocks.

**Lemma:** Assume wages equal marginal products plus transfers, \( B_{ict} \neq 0 \). But assume that these take the form of zero-sum transfers across individuals so that they average to zero

\[ \frac{1}{T} \sum_{t=1}^{T} \frac{1}{C_t} \sum_{c=1}^{C_t} \frac{1}{N_{ct}} \sum_{i=1}^{N_{ct}} B(s_{ict}, x_{ict}) = 0. \]  

(12)

and that no transfers are received by individuals with zero experience or schooling \( B(0,0) = 0 \). Then our procedure for estimating human capital stocks in sections 3 and 4 still correctly identifies the correct aggregate human capital stock, \( H \), even though estimates of individual human capital stocks, \( h_{ict} \) are biased. If, in addition, the economy has a stationary age distribution and schooling choices are stationary, the condition under which aggregate human capital stocks are identified, (12), has the alternative interpretation of zero-sum transfers in expectation over the lifecycle of a given individual.

**Proof:** Combining equations (9) to (11), one can show that the analogue of our estimating equation (4) is now

\[ \log y_{ict} = \log \bar{w} + m(s_{ict}, x_{ict}) + b(s_{ict}, x_{ict}) + \gamma_t + \psi_c + \varepsilon_{ict} \]

where

\[ b(s_{ict}, x_{ict}) = \log \left( 1 + \frac{B(s_{ict}, x_{ict})}{\bar{w} \exp(m(s_{ict}, x_{ict}))} \right). \]

Our identification strategy in section 3.1 still correctly identifies the intercept \( \log \bar{w} \) because \( B(0,0) = 0 \) and hence \( b(0,0) = 0 \). But we can no longer separately identify the functions \( m \) and \( b \). The slope of the experience-earnings profile will therefore also be biased. And we would obtain biased estimates of individual human capital stocks

\[ \hat{h}_{it} = \exp(m(s_{it}, x_{it}) + b(s_{it}, x_{it})) \neq \exp(m(s_{it}, x_{it})) = h_{it} \]

However, this is not a problem when one is only interested in estimating the aggregate human capital
stock. To see this write the estimated human capital stocks as

\[ \hat{h}_{ict} = \exp(m(s_{ict}, x_{ict}) + b(s_{ict}, x_{ict})) = \exp(m(s_{ict}, x_{ict})) \left(1 + \frac{B(s_{ict}, x_{ict})}{w \exp(m(s_{ict}, x_{ict}))}\right) = h_{ict} + \frac{B(s_{ict}, x_{ict})}{w}. \]

The aggregate human capital stock is then simply estimated as the average over all individual estimated human capital stocks. Using that transfers average to zero, (12), it is easy to see that this procedure correctly identifies the aggregate human capital stocks

\[
\hat{H} = \frac{1}{T} \sum_{t=1}^{T} \frac{1}{C_t} \sum_{c=1}^{C_t} \frac{1}{N_{ct}} \sum_{i=1}^{N_{ct}} \hat{h}_{ict} = \frac{1}{T} \sum_{t=1}^{T} \frac{1}{C_t} \sum_{c=1}^{C_t} \frac{1}{N_{ct}} \sum_{i=1}^{N_{ct}} h_{ict} + \frac{1}{T} \sum_{t=1}^{T} \frac{1}{C_t} \sum_{c=1}^{C_t} \frac{1}{N_{ct}} \sum_{i=1}^{N_{ct}} \frac{B(s_{ict}, x_{ict})}{w}.
\]

This proves the first part of the Lemma. If the economy additionally has a stationary age distribution and stationary schooling choices, then the fraction of individuals with a given experience and schooling level is also the probability of an individual living up to the age where she attains that same experience and schooling levels. Therefore condition (12) can be interpreted as the requirement that transfers sum to zero in expectation over the lifecycle of a given individual. □

Numerical Example The following numerical example illustrates which departures from the assumption that wages equal marginal products of labor. For simplicity abstract from cohort and year effects and set \( \gamma_t = \psi_c = 0 \). Assume that human capital production has the quintic functional form

\[ h_i = \exp(\theta s_i + \phi_1 x_i + \phi_2 x_i^2 + \phi_3 x_i^3 + \phi_4 x_i^4 + \phi_5 x_i^5) \]

where we use the \( \phi_k \) estimated for the US in section 3.2. Transfers are assumed to depend on experience in a linear-quadratic fashion, \( B_i = \beta_1 x_i + \beta_2 x_i^2 \), and the coefficients \( \beta_1 \) and \( \beta_2 \) are such that (12) is satisfied.\(^{33}\) Panel (a) of Figure A.4 plots two examples of such transfer functions. Panel (b) plots the implied experience-earnings profiles (dashed lines) and the underlying correct human capital profile for sake of comparison (solid line). If our empirical exercise estimated either of the dashed experience-earnings profiles, we would still identify the correct aggregate human capital stock, that is the one corresponding to the solid experience-earnings profile.

\(^{33}\)We assume for simplicity that experience is distributed uniformly between 0 and \( \bar{x} = 45 \). Therefore, \( \beta_1 \) and \( \beta_2 \) satisfy

\[ \int_0^{\bar{x}} [\beta_1 x + \beta_2 x^2] \frac{1}{\bar{x}} dx = 0 \quad \Leftrightarrow \quad \beta_1 + \beta_2 \frac{2 \bar{x}}{3} = 0. \]

Further, with our functional form assumptions we have that experience-wage profiles are

\[ \log y_i = \log \bar{w} + \theta s_i + \phi_1 x_i + \phi_2 x_i^2 + \phi_3 x_i^3 + \phi_4 x_i^4 + \phi_5 x_i^5 + b(x_i) + \varepsilon_i \]

\[ b(x_i) \equiv \log \left(1 + \frac{\beta_1 x_i + \beta_2 x_i}{\exp(\phi_1 x_i + \phi_2 x_i^2 + \phi_3 x_i^3 + \phi_4 x_i^4 + \phi_5 x_i^5)}\right).
\]
In section 5.1 we documented that experience-earnings profiles for individuals with more than ten years of schooling are flatter than those for less than ten years of schooling. This is true even though the opposite pattern holds for age-earnings profiles. The purpose of this Appendix is to argue that this pattern arises relatively mechanically. In particular, going back and forth between age-earnings and experience-earnings profiles mainly involves a rescaling of the axes that differentially affects the two schooling categories and hence results in the observed pattern. We explain in detail how this works for the United States – other countries look similar.

Figure A.5(a) plots the age-earnings profile for the two different schooling categories. The age-earnings profiles for highly educated individuals are steeper than those of their uneducated counterparts. This is consistent with the findings documented in (Carroll and Summers, 1991, Figures 10.7a and 10.8a; Guvenen, 2007, Figure 2; Kambourov and Manovskii, 2009, Figures 3, 6, 8 and 10). Highly educated individuals start working later and at a higher starting wage. Figure A.5(b) changes the x-axis from age to experience. Figure A.5(c) additionally normalizes the logarithm of the starting wage to zero for the two schooling categories, that is rescales the y-axis. It can be seen that this rescaling of the axes generates the reversal in the relative slopes of age-earnings vis-à-vis experience earnings profiles.
Table 1: Dispersion of Aggregate Human Capital Stocks

<table>
<thead>
<tr>
<th>Human Capital Measure</th>
<th>Dispersion Measure</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
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<tr>
<td>Schooling</td>
<td>0.142</td>
<td>1.90</td>
</tr>
<tr>
<td>Experience</td>
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<td>1.97</td>
</tr>
<tr>
<td>Schooling + Experience</td>
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<td>3.74</td>
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Table 2: Dispersion of Aggregate Human Capital Stocks

<table>
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<th>Human Capital Measure</th>
<th>Dispersion Measure</th>
<th></th>
</tr>
</thead>
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<tr>
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<td>Cohort Controls</td>
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<tr>
<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
</tr>
<tr>
<td>Schooling</td>
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</tr>
<tr>
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Table 3: Development Accounting

<table>
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<th>Human Capital Measure</th>
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<tr>
<td></td>
<td>Success1</td>
<td>Success2</td>
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<tr>
<td>Schooling</td>
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<tr>
<td>Experience</td>
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<td>0.46</td>
</tr>
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<td>Schooling + Experience</td>
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<td>0.70</td>
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Table 4: Development Accounting (Robustness)

<table>
<thead>
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<th>Human Capital Measure</th>
<th>Success Measure</th>
<th></th>
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</thead>
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<tr>
<td></td>
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<td>Cohort Controls</td>
</tr>
<tr>
<td></td>
<td>Success1</td>
<td>Success2</td>
</tr>
<tr>
<td>Schooling</td>
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<td>0.46</td>
</tr>
<tr>
<td>Experience</td>
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<td>0.46</td>
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<tr>
<td>Schooling + Experience</td>
<td>0.61</td>
<td>0.72</td>
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Table 5: Counterfactual: US Employment Share in Agriculture

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<tr>
<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
</tr>
<tr>
<td>Data</td>
<td>0.110</td>
<td>1.97</td>
</tr>
<tr>
<td>Counterfactual</td>
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<td>1.92</td>
</tr>
<tr>
<td>Fraction due to Composition Effect</td>
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<td>0.05</td>
</tr>
<tr>
<td>Fraction due to Within-Sector Diffs</td>
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<td>0.95</td>
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</table>

Table 6: Counterfactual: US Share of Workers with Low Schooling

<table>
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</tr>
</thead>
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<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
</tr>
<tr>
<td>Data</td>
<td>0.110</td>
<td>1.97</td>
</tr>
<tr>
<td>Counterfactual</td>
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<td>-0.28</td>
</tr>
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<td>Fraction due to Within-Sector Diffs</td>
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<td>1.28</td>
</tr>
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</table>
Figure 1: Returns to Experience in Richest Quartile of Our Data

Figure 2: Returns to Experience in 2nd Quartile of Our Data
Figure 3: Returns to Experience in 3rd Quartile of Our Data

Figure 4: Returns to Experience in Poorest Quartile of Our Data
Figure 5: Returns to Experience for Each Year of Experience – Cross-Sectional Estimates

Figure 6: Returns to Experience for Each Year of Experience – Cross-Sectional Estimates with 99% Confidence Intervals
Figure 7: Height of Profile at 20 Years of Experience

Figure 8: Mincer Estimates of Returns to Experience – Cross-Sectional Estimates
Figure 9: Quintic Estimates of Returns to Experience – Cross-Sectional Estimates

Figure 10: Returns to Experience for Each Year of Experience – Control for Time Effects
Figure 11: Returns to Experience for Each Year of Experience – Control for Cohort Effects

Figure 12: Quintic Estimates of Returns to Experience– Control for Time Effects
Figure 13: Quintic Estimates of Returns to Experience – Control for Cohort Effects

Figure 14: Quintic Estimates of Returns to Experience – Control for Both Cohort Time Effects
Figure 15: Height of Profile at 20 Years of Experience with Linear-Quadratic Mincerian Specification
Figure 16: Experience Histograms

(a) USA

(b) Canada

(c) Brazil

(d) Mexico

(e) India

(f) China (Urban)
Figure 17: Differences in Experience Across Countries

Figure 18: Implied Human Capital from Experience and Per Capita GDP (Based on cross-sectional estimates of the returns to experience)
Figure 19: Implied Human Capital from Experience and Per Capita GDP (Based on estimates of the returns to experience with cohort controls)

Figure 20: Implied Human Capital from Experience and Per Capita GDP (Based on estimates of the returns to experience with time controls)
Figure 21: Implied Human Capital from Experience and Per Capita GDP (Based on estimates of the returns to experience with both cohort time controls)

Figure 22: Implied Human Capital from Experience and Per Capita GDP (Based on cross-sectional linear-quadratic Mincer estimates of the returns to experience)
Figure 23: Employment Share of Agriculture

Figure 24: Height of Profiles at 20 Years of Experience: Agriculture vs. Non-Agriculture
Figure 27: Height of Profiles at Twenty Years of Experience: High Schooling vs. Low Schooling

Figure 28: Counterfactual: US Share of Workers with Low Schooling
<table>
<thead>
<tr>
<th>Country</th>
<th>Fully Flexible Estimation of Returns to Experience</th>
<th>Quintic</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Cross-Section (1)</td>
<td>Time Controls (2)</td>
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<tr>
<td>Argentina</td>
<td>0.52</td>
<td>0.53</td>
</tr>
<tr>
<td>Australia</td>
<td>0.69</td>
<td>0.69</td>
</tr>
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<td>Bangladesh</td>
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<tr>
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<td>0.56</td>
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<tr>
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<td>0.90</td>
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<tr>
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<td>0.61</td>
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<tr>
<td>Germany</td>
<td>0.87</td>
<td>0.87</td>
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<tr>
<td>India</td>
<td>0.49</td>
<td>0.47</td>
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<td>Indonesia</td>
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<td>Italy</td>
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<td>0.91</td>
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<td>Jamaica</td>
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<td>Peru</td>
<td>0.57</td>
<td>0.57</td>
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<tr>
<td>Puerto Rico</td>
<td>1.13</td>
<td>1.12</td>
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<tr>
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<td>Thailand</td>
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<td>United Kingdom</td>
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<td>Uruguay</td>
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<tr>
<td>Vietnam</td>
<td>0.41</td>
<td>0.40</td>
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</table>

Notes: The aggregate human capital estimates in columns (1)-(3) are based on returns to experience estimates from fully flexible specifications (e.g., years of experience dummies). Those in columns (4)-(7) are based on a quintic specification. In columns (2) and (5), we include controls for calendar year dummy variables. In columns (3) and (6), we include categorical controls for birth cohorts (e.g., dummy variables indicating ten-year intervals for birth year). In column (7) we include both birth cohort and calendar year dummies.
Table A.2: Aggregate Total Human Capital

<table>
<thead>
<tr>
<th>Country</th>
<th>Our Total Human Capital (Schooling + Experience)</th>
<th>Caselli's Schooling Human Capital + Our Experience Human Capital</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td></td>
<td>No Experience</td>
<td>Cross-Section</td>
</tr>
<tr>
<td>Argentina</td>
<td>0.87</td>
<td>0.40</td>
</tr>
<tr>
<td>Australia</td>
<td>0.82</td>
<td>0.51</td>
</tr>
<tr>
<td>Bangladesh</td>
<td>0.37</td>
<td>0.17</td>
</tr>
<tr>
<td>Brazil</td>
<td>0.92</td>
<td>0.49</td>
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<tr>
<td>Canada</td>
<td>0.84</td>
<td>0.73</td>
</tr>
<tr>
<td>Chile</td>
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<tr>
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<td>0.26</td>
</tr>
<tr>
<td>Indonesia</td>
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<td>0.33</td>
</tr>
<tr>
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<td>0.47</td>
</tr>
<tr>
<td>Jamaica</td>
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<td>0.42</td>
</tr>
<tr>
<td>Korea, Rep.</td>
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<td>0.51</td>
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<tr>
<td>Mexico</td>
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</tr>
<tr>
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<td>Peru</td>
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<td>0.66</td>
</tr>
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<td>Puerto Rico</td>
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<td>Taiwan</td>
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<tr>
<td>Thailand</td>
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<tr>
<td>United Kingdom</td>
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<tr>
<td>Uruguay</td>
<td>0.51</td>
<td>0.29</td>
</tr>
<tr>
<td>Vietnam</td>
<td>0.39</td>
<td>0.15</td>
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</tbody>
</table>

Notes: The aggregate human capital estimates in columns (1)-(5) are based on a quintic specification. Those in column (6) are from Caselli (2005), taking into account only years of schooling. Those in columns (7)-(10) are our estimates of experience human capital stocks (estimated using a quintic specifications) multiplied by Caselli’s schooling human capital from column (6). In columns (3) and (8), we include controls for calendar year dummy variables. In columns (4) and (9), we include categorical controls for birth cohorts (e.g., dummy variables indicating ten-year intervals for birth year). In columns (5) and (10) we include both birth cohort and calendar year dummies.
Table A.3: Development Accounting

<table>
<thead>
<tr>
<th>Country</th>
<th>Data from Caselli (2005)</th>
<th>Success 2 as defined in Caselli (2005), Relative to US</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Y</td>
<td>K</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Argentina</td>
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<td>0.39</td>
</tr>
<tr>
<td>Australia</td>
<td>0.81</td>
<td>0.95</td>
</tr>
<tr>
<td>Bangladesh</td>
<td>0.11</td>
<td>0.05</td>
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<tr>
<td>Brazil</td>
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<td>0.31</td>
</tr>
<tr>
<td>Canada</td>
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<td>0.98</td>
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<tr>
<td>Chile</td>
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<td>China</td>
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<tr>
<td>France</td>
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<td>1.08</td>
</tr>
<tr>
<td>Germany</td>
<td></td>
<td></td>
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<tr>
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<tr>
<td>Mexico</td>
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<td>0.35</td>
</tr>
<tr>
<td>Nicaragua</td>
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<td>0.08</td>
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<tr>
<td>Panama</td>
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<td>0.25</td>
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<tr>
<td>Peru</td>
<td>0.18</td>
<td>0.18</td>
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<tr>
<td>Puerto Rico</td>
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<tr>
<td>Russian Federation</td>
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</tr>
<tr>
<td>Switzerland</td>
<td>0.77</td>
<td>1.27</td>
</tr>
<tr>
<td>Taiwan</td>
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<tr>
<td>Thailand</td>
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<td>0.30</td>
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<tr>
<td>United Kingdom</td>
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<td>0.70</td>
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<tr>
<td>Uruguay</td>
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<td>0.24</td>
</tr>
<tr>
<td>Vietnam</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Columns (1) and (2) report GDP per worker and physical capital stocks from Caselli (2005), that we use as inputs into our accounting exercise. Columns (3)-(12) report Caselli’s “success2” measure which is the fraction of a given country’s income gap to the US that can be explained by physical and human capital. Columns (3)-(7) use as inputs our aggregate human capital stocks estimated with a quintic specification. Column (8) uses those from Caselli (2005), taking into account only years of schooling. Those in columns (9)-(12) are our estimates of experience human capital stocks (estimated using a quintic specifications) multiplied by Caselli’s schooling human capital from columns (8). In columns (5) and (10), we include controls for calendar year dummy variables. In columns (6) and (11), we include categorical controls for birth cohorts (e.g., dummy variables indicating ten-year intervals for birth year). In columns (7) and (12) we include both birth cohort and calendar year dummies.
Table A.4: Alternative Sample Restriction: Male Workers

<table>
<thead>
<tr>
<th>Human Capital Measure</th>
<th>Dispersion Measure</th>
<th>Success Measure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.122</td>
<td>1.72</td>
</tr>
<tr>
<td>Experience</td>
<td>0.142</td>
<td>2.22</td>
</tr>
<tr>
<td>Schooling + Experience</td>
<td>0.399</td>
<td>3.81</td>
</tr>
</tbody>
</table>

Table A.5: Alternative Sample Restriction: Male Private Full-time Workers

<table>
<thead>
<tr>
<th>Human Capital Measure</th>
<th>Dispersion Measure</th>
<th>Success Measure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.128</td>
<td>1.67</td>
</tr>
<tr>
<td>Experience</td>
<td>0.169</td>
<td>2.00</td>
</tr>
<tr>
<td>Schooling + Experience</td>
<td>0.425</td>
<td>3.32</td>
</tr>
</tbody>
</table>

Table A.6: Alternative Definition of Potential Experience: Start Work at Age 6

<table>
<thead>
<tr>
<th>Human Capital Measure</th>
<th>Dispersion Measure</th>
<th>Success Measure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.140</td>
<td>1.79</td>
</tr>
<tr>
<td>Experience</td>
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<td>1.70</td>
</tr>
<tr>
<td>Schooling + Experience</td>
<td>0.298</td>
<td>3.08</td>
</tr>
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</table>

Table A.7: Alternative Definition of Potential Experience: Start Work at Age 6 - Male Private Full-time Workers

<table>
<thead>
<tr>
<th>Human Capital Measure</th>
<th>Dispersion Measure</th>
<th>Success Measure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.125</td>
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</tr>
<tr>
<td>Experience</td>
<td>0.146</td>
<td>1.88</td>
</tr>
<tr>
<td>Schooling + Experience</td>
<td>0.367</td>
<td>2.98</td>
</tr>
</tbody>
</table>

Table A.8: Alternative Definition of Potential Experience: Start Work at Age 15

<table>
<thead>
<tr>
<th>Human Capital Measure</th>
<th>Dispersion Measure</th>
<th>Success Measure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Var(ln H)</td>
<td>90-10 Ratio</td>
</tr>
<tr>
<td>Schooling</td>
<td>0.139</td>
<td>1.90</td>
</tr>
<tr>
<td>Experience</td>
<td>0.118</td>
<td>2.01</td>
</tr>
<tr>
<td>Schooling + Experience</td>
<td>0.369</td>
<td>3.80</td>
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</table>
Figure A.1: Fraction of Individuals with Positive Wage Earnings by Age

Figure A.2: Returns to Experience for Different Degrees of Age Heaping
Figure A.3: Returns to Experience from Census and CPS data

Figure A.4: Acceptable Violations of \( w = MPL \)

(a) Transfers

(b) Implied Experience-Earnings Profiles
Figure A.5: From Age-Earnings to Experience-Earnings Profiles

(a) Age-Earnings Profile (Level)  
(b) Experience-Earnings Profile (Level)  
(c) Experience-Earnings Profile (Relative)

Figure A.6: Hours at Twenty Years of Experience Across Countries